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XXVI. *Experiments on Ammonia, and an Account of a new Method of Analyzing it, by Combustion with Oxygen and other Gases; in a Letter to Humphry Davy, Esq. Sec. R. S. &c. from William Henry, M. D., F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester.**

MY DEAR SIR,

I SHOULD sooner have communicated the account, which you are so good as to request, of my further experiments on the decomposition of ammonia, if I had not been anxious to obtain, by frequent and careful repetition of them, results not affected by any of those numerous causes of error, which easily insinuate themselves into processes of so much delicacy. You have already been informed that the fact, which I lately mentioned to you, (tending to prove the existence of oxygen as an element of the volatile alkali, by the discovery of oxygen gas in the products of its analysis) is not entitled to confidence, owing to the admission of a small quantity of atmospherical air, in a way which was not at all suspected. Frequent repetitions of the same process, under circumstances wholly unobjectionable, have fully satisfied me, that no portion whatsoever of oxygen gas is evolved by electricity from ammonia, even when, by means of an apparatus constructed for the

* This letter in its original form, was read to the Society, May 18th, 1809, some new observations were added and some corrections furnished by the author, in consequence of subsequent experiments made in June; it was transmitted to the Secretary for publication July 10.

purpose, the only metallic surface, exposed to the gas, consists of the sections of two platina wires, each $\frac{1}{50}$ of an inch in diameter, the wires themselves being inclosed in glass tubes which are sealed hermetically round them, and then ground away, so as to expose only the points. Nor does any difference in the nature of the products arise from electrifying the gas either under increased or diminished pressure, the latter of which, it appeared to me probable, from the known influence of elasticity in impeding the combination of gaseous bases, might prevent the oxygen of the alkali from uniting with hydrogen to form water, and occasion the expansion of both into the state of gas.

Having failed, therefore, to acquire, in this way, proof of the existence of oxygen in the volatile alkali, I was next led to seek for some unequivocal mode of evincing the production of water by the same operation ; a fact, which would be scarcely less satisfactory in establishing oxygen to be one of its constituents, than the actual separation of oxygen gas. The most careful observation of ammonia, during and after the agency of electricity, does not discover the smallest perceptible quantity of moisture. In order, therefore, to subject the gas to a satisfactory test, I had recourse to the following contrivance. Ammoniacal gas, I had previously found, may be so far desiccated by exposure to caustic potash, as to shew no traces of condensed moisture, on the inner surface of a thin glass vessel containing it, when exposed to a cold of 0° FAHRENHEIT ; though the recent gas, by the same treatment, is made to deposit water in the state of a thin film of ice. A glass globe, of the capacity of between two and three cubical inches, was filled with gaseous ammonia, which was then

dried by sticks of pure potash, fastened to pieces of steel wire, so that they could be withdrawn, after having exerted their full action. This point of dryness was ascertained by applying æther, or a mixture of snow and salt, to the outside of the globe. By means of a peculiar apparatus, the gas was next strongly electrified, and the cooling power was again applied to the outer surface of the globe.

In the first trials, that were made with this apparatus, water certainly seemed to have been formed by the electrization of the alkaline gas; for the same portion of gas, which was not affected by a freezing mixture before the process, gave evident signs of condensed moisture, when the cooling power was applied after long continued electrization. The appearance was not only quite satisfactory to myself, but to Mr. DALTON, and several other chemical friends, to whom I shewed the experiment. Finding, however, that the appearance varied as to its degree, I was induced to repeat the process with redoubled precaution; filling the globe, previously heated, with hot mercury, and drying not only the quicksilver, but the iron cistern which contained it, by exposure to long continued heat. The electrified gas now betrayed no signs of moisture on the application of a temperature 20° of FAHRENHEIT; and gave only the smallest perceptible traces, by a cold of 0° or a few degrees below. I cannot help suspecting, therefore, that the moisture, manifested in the earlier experiments, was derived from the mercury or from some extraneous source, and was not generated by the action of electricity.*

* It may be objected, I am aware, that as the gases produced from ammonia are nearly double its original bulk, they may hold in combination any water that may

The avidity with which ammonia retains moisture, and again absorbs it when artificially dried, is very remarkable. A confined quantity of common air may be completely desiccated, in the space of a few minutes, by pure potash, or by muriate of lime; so that no ice shall appear in the inner surface of the containing vessel, when exposed to a cold of -26° of FAHRENHEIT. But ammonia requires exposure during some hours to potash, to stand the test even of 0° FAHRENHEIT; and a single transfer of the dried gas, through the mercury of a trough in ordinary use, again communicates moisture to it. Muriatic acid gas, freed merely from visible moisture, deposits no water at the temperature of 26° FAHRENHEIT. This is probably owing to its strong affinity for water; for electricity, after the full action of muriate of lime, evolves, as I have lately ascertained, about $\frac{1}{3}$ th its bulk of hydrogen gas, the recent muriatic acid gas giving about $\frac{1}{14}$ th after the same treatment.*

have been generated by electricity. But though this supposition may explain the non-appearance of *visible moisture*, it does not account for the inefficiency of a powerful cooling cause to discover traces of watery vapour: for this is a test which renders apparent very minute quantities of water in gases.

* In a course of experiments, which I have described in the *Philosophical Transactions* for 1800, it appeared that muriatic acid gas, after being dried by muriate of lime, gave nearly as much hydrogen by electrization, as gas which had not been thus exposed. I was not however aware, at that time, of the extreme caution necessary in experiments of this kind; and was satisfied with transferring the acid gas from a large vessel, in which it had been dried, into the electrizing tube, a mode of proceeding which I now find to be quite inadmissible. The action of muriate of lime, which has undergone fusion, on muriatic acid gas, is rendered very sensible, when considerable quantities are used, by the evolution of much heat, and by a diminution of the volume of the gas. Ammonia, also, is contracted in bulk by dry caustic potash. Muriate of lime cannot be employed for its desiccation, since this substance rapidly absorbs

From the average of a great number of experiments on the decomposition of ammonia by electricity, I was for some time led to believe, that you had rather under-stated the proportion of permanent gases obtainable from it by this process, (viz. 108 measures of permanent gas, from 60 of ammonia or 180 from 100). For the most part, I had found the bulk of ammonia to be doubled by decomposition, even when the gas was previously dried with extreme care. In one instance, a small bit of dry potash was left in the tube, along with the ammonia, during electrization, with the view of its absorbing water, which I supposed, at that time, to be generated by the process. In this case, 59 measures (each = 10 grains of mercury) became 115. The following table shews the expansion of various quantities of ammonia.

Exp.

1.	60 measures of ammonia, gave permanent gas	112
2.	60 - - - - -	120
3.	59 (potash being left in the tube) - -	115
4.	55 - - - - -	115
5.	75 (under the pressure of half an atmosphere)	150
6.	65 - - - - -	130
7.	65 - - - - -	130
8.	53 (one of the conductors being of steel wire)	106
<hr/>		<hr/>
	492	978

and 492 : 978 :: 100 : 198,78. These proportions, you will find, correspond very nearly with those long ago stated

the alkaline gas, even when the gas has been previously exposed to quick-lime. In this case, the ammonia attracts a portion of muriatic acid from the earthy salt, agreeably to the law of affinity, which has been so ably illustrated by BERTHOLLET.

by BERTHOLLET,* who converted 17 measures of ammonia by electrization, into 33 measures of permanent gas, which is at the rate of 194 from 100. Having lately, however, carried on the process with the observance of additional precaution, (the mercury being first boiled in the tube, before admitting the ammonia, and still remaining hot when the gas was passed up), I have obtained from the alkali less than double its volume of permanent gas, viz. 280 measures from 155, or at the rate of 180,6 from 100. The variableness of the first set of results arises, I believe, from the uncertainty of the quantity of ammonia decomposed. For if the smallest portion of moisture remain in the tube, a little ammoniacal gas will be absorbed, and will be slowly given out again as the electrization goes on, thus rendering the actual quantity submitted to experiment greater than appears. It is probable, also, from a fact which I shall afterwards state, that mercury itself, unless when heated, may absorb a small portion of alkaline gas.

The proportion of the hydrogen and nitrogen gases to each other in the products of ammonia decomposed by electricity, I am satisfied, by recent experiments (June, 1809) is as nearly as possible what you have determined, viz. 74 measures of hydrogen gas to 26 of nitrogen. The nearest approximation I have made to these numbers is 73,75 to 26,25. Our only method of analyzing mixtures of these two gases, (viz. by combustion with a redundancy of oxygen) is not, I believe, sufficiently perfect to afford a nearer coincidence.

The extreme labour and tediousness of the decomposition of ammonia by electricity, influenced me to attempt the discovery of a shorter and more summary method of analysis. The

* *Journal de Physique*, 1786, ii. 176.

most obvious one, was its decomposition by oxymuriatic acid gas; but this plan was abandoned, from the impossibility of confining both the gases by any one fluid; since water acts powerfully on the one, and mercury on the other. But a mixture of oxygen and ammoniacal gases more than answered my expectations. When mingled in proper proportions, these gases, I have ascertained, may be detonated over mercury by an electric spark; exactly like a mixture of vital and inflammable air; and the results of the process, with due attention to the circumstances, which will soon be stated, afford an easy and precise method of analyzing, in the space of a few minutes, considerable quantities of the volatile alkali. With a greater proportion of pure oxygen gas* to ammonia than that of three to one, or of ammonia to oxygen than that of three to 1.4, the mixture ceases to be combustible. When the proportions best adapted to inflammation are used, oxygen gas may be diluted with six times its bulk of atmospherical air, without losing its property of burning ammonia.

Atmospherical air alone does not, however, inflame with ammonia, in any proportion that I have yet tried; though, by long continued electrization with air, ammonia is at length decomposed; its hydrogen uniting with the oxygen of the air and forming water, while the nitrogen of both composes a permanent residuum. Forty-five measures of ammonia being electrified with eighty-six of common air, the total 131 became 136, and 132 after being washed with water. Of 17.2 measures of oxygen, contained in the 86 measures of air at the outset, only 2.9 were left, and these, also, would probably have disappeared by continuing the operation. If a mix-

* Containing only three or four per cent. nitrogen gas.

ture of ammonia and atmospheric air, each previously dried by caustic potash and then electrified, be examined, the production of water is made sufficiently apparent on applying ether to the containing vessel. In subjecting ammonia, therefore, to this test of the generation of water by electricity, the purity of the gas from atmospheric air should be carefully determined.*

The products of the combustion of ammonia with oxygen vary essentially, according to the proportion of the gases which are employed. If the oxygen gas exceed considerably the ammonia (that is, if its volume be double or upwards) the ammonia entirely disappears; and no gases remain, but a mixture of nitrogen with the redundant oxygen. The moment the detonation is completed, a dense cloud appears,† and soon afterwards settles into a white incrustation on the inner surface of the tube. The quantity of this substance, which is produced, is too minute for analysis; but its characters resemble those of nitrate of ammonia, the acid ingredient of which is probably generated by the action of oxygen on the nitrogen of one part of the volatile alkali. Accordingly, when the excess of oxygen is removed by sulphuret of lime,

* The result of this experiment shews, moreover, that even supposing oxygen to be a constituent of ammonia, we are not to expect its evolution, in a separate form, by electricity; since, when electrified with ammoniacal gas, oxygen gas is deprived of its elastic form, and its base is condensed into water, by union with nascent hydrogen evolved from the alkali.

† In some cases I have observed, that when the cloud does not occur immediately, it may be made to appear by agitating the quicksilver contained in the detonating tube. This is probably owing to the disengagement of some ammonia, which had lodged in the mercury. The fact confirms what I have already suggested, respecting the cause of the variable proportion of gases, evolved from ammonia by electricity.

the nitrogen generally falls short of the proportion, which ought to accrue from a given weight of ammonia; and hence it is scarcely possible to attain, when a considerable excess of oxygen is used, an accurate analysis of the volatile alkali.

When, on the contrary, the ammonia exceeds considerably the oxygen gas, no production of nitrous acid appears to take place; for the residue, after detonation, is quite free from cloudiness. It is remarkable, however, that ammonia when fired, in certain proportions, with less oxygen than is required to saturate its combustible ingredient, is nevertheless completely decomposed. Part of its hydrogen is sufficient for the saturation of the oxygen; and the remaining hydrogen, and the whole nitrogen of the ammonia, together with that existing as an impurity in the oxygen employed, remain in a gaseous state, and compose a mixture, which may be inflamed by adding a second quantity of oxygen gas, and passing an electric spark.* In this way all the hydrogen of the volatile alkali may be saturated with oxygen, and condensed into water; and the whole of the nitrogen may be obtained as a final result of the process. After determining the amount of the oxygen, consumed both in the first and second combustions, it is easy to calculate the quantity of hydrogen, in the saturation of which it has been employed; for when no nitrous acid has been formed, the hydrogen will be, pretty exactly, double in volume the oxygene which has been expended.

* This is analogous to what happens, when ether, alcohol, or any of the aëriform compounds of carbon and hydrogen, are exploded with a deficient proportion of oxygen; for much of the hydrogen is found in the residuum in the state of gas, and again becomes susceptible of combustion after the addition of a second quantity of oxygen. (See Mr. CRUICKSHANK'S excellent papers in the 5th Volume of NICHOLSON'S Journal, 4to.)

These general observations will tend to render the following experiments more intelligible. They may be divided into two classes, 1st, those in which ammonia was fired with an excessive proportion of oxygen; and 2dly, those in which the oxygen, used in the first combustion, was insufficient, or barely adequate, to saturate the whole hydrogen of the alkali.

I. *Decomposition of Ammonia by an Excess of oxygen Gas.*

Twenty-two measures and a third of ammonia were mixed with $44\frac{2}{3}$ oxygen containing 43 of pure gas. The total 67 became 34 when exploded. Water did not produce any farther diminution, but sulphuret of lime left only 8 measures. Now, $34 - 8 = 26$ shews the quantity of oxygen gas, which escaped condensation; and this, deducted from the original quantity (43) gives 17 measures for the amount of the oxygen expended. The last number 17, being multiplied by 2, gives 34 for the hydrogen apparently consumed. The final residue $8 - 1.66$ (the nitrogen introduced by the oxygen gas) $= 6.34$ is the nitrogen obtained from $22\frac{1}{3}$ of ammonia; and if to this the hydrogen be added, 40.34 measures of permanent gas will be the total result. Hence 100 measures of the gas producible from ammonia, should contain 84.29 hydrogen and 15.71 nitrogen; numbers too remote from those, which have been already assigned, to be considered even as approximations to the truth. The error arises from the combination of oxygen, during combustion, not only with the hydrogen, but with the nitrogen of the alkali, the latter of which consequently appears deficient, and the former proportionably in excess.

Frequent repetitions of this combustion, with a considerable excess of oxygen gas, continued to give a deficient proportion of nitrogen; and as no accurate conclusions can be drawn from experiments of this kind, I shall proceed to those of the second class.

II. *Experiments, in which Ammonia was fired with a deficient Proportion of oxygen Gas.*

Sixty-three measures of ammonia were exploded over mercury with 33 of oxygen gas containing one of nitrogen. The total 96, when fired by an electric spark, were diminished to 57 measures, which were not contracted any farther by successive agitation with water, and with sulphuret of lime. The whole of the ammonia, therefore, was decomposed; and all the oxygen had entered into combination with the hydrogen of the alkali. The residuary 57 measures were mingled with 40 measures of the same oxygen gas, and detonated by an electric spark; after which the total, 97, were reduced to 60. The diminution, therefore, was 37 measures; and as two thirds of this number may be ascribed to the condensation of hydrogen gas, the residuary 57 must have been composed of 24.66 hydrogen, and 32.34 nitrogen. The oxygen expended, also, was 32 in the first combustion, + 12.33 in the second = 44.33; and this number, being doubled, gives 88.66 for the whole hydrogen saturated, supposing it to be in the state of hydrogen gas. But from the above quantity of nitrogen (32.34 measures) we are to deduct one measure, with which the 33 measures of oxygen were contaminated; and the remainder 31.34 shews the number of measures of nitrogen, resulting from 63 measures of ammonia. The total amount of gases

obtained is $31.34 + 88.66 = 120$; and the proportion of the hydrogen by volume to that of the nitrogen, as 73.88 to 26.12.

To avoid the tediousness of similar details, I shall state, in the form of a table, the results of a few experiments out of a number of others, all of which had, as nearly as could be expected, the same tendency. The sixth experiment in the table is the one which has been just described.

No. of Exp.	Meas. of Ammon. decomposed.	Meas. of Oxygen saturated	Meas. of Hydrogen estimated.	Meas. of Nitrogen obtained.	Hence 100 Meas. of Ammonia.		Permanent Gas contains in 100 Measures.	
					Take Oxygen.	Give Gas.	Hydr.	Nitr.
1	72	47.5	95	37	66	183	72	28
2	95	64	128	46	67.5	183.3	73.5	26.5
3	100	72.2	144.4	54	72.2	198.5	72.8	28.2
4	74	51.7	103.4	37	69.8	189	73.6	27.4
5	49	33.7	67.4	25.3	68.7	180.2	72.7	27.3
6	63	44.3	88.6	31.3	70.3	193.5	73.9	26.1

From an attentive examination of the foregoing table, it will appear that the results are not perfectly uniform, though perhaps as much as can be expected from the nature of the experiments. Thus the proportion of permanent gases to the ammonia decomposed (the nitrogen being actually measured, and the hydrogen estimated by doubling the oxygen expended) may be observed to differ considerably; the highest product being $198\frac{1}{2}$, and the lowest 180.2, from 100 of ammonia.

There can scarcely be a doubt, however, that this want of coincidence is owing to the same cause, as that which I have already assigned for the variable proportions of permanent gas, which are obtained from equal quantities of ammonia by electrization. And, accordingly, I have found that the evolved gases, as ascertained by combustion, bear the smallest proportion to the ammonia, when most pains have been taken to obviate the presence of moisture. The lowest number, therefore, is to be assumed as most correct; but other circumstances being considered, I believe the second experiment furnishes the most accurate data for determining the composition of ammonia. The same explanation will apply to the different proportions of oxygen gas required for the saturation of 100 measures of ammonia, the variation no doubt arising from the uncertainty of the quantity of alkaline gas which is actually burned. The proportion of oxygen to ammonia, which I believe to be nearest the truth, and most precisely necessary for mutual saturation, is that resulting from the second experiment, *viz.* $67\frac{1}{2}$ measures of oxygen gas to 100 of ammonia, or 100 of the former to 148 of the latter.

It may be observed, also, by comparing the numbers in the two last columns of the table, that the hydrogen and nitrogen gases do not uniformly bear the same proportions to each other. Notwithstanding all the labour I have bestowed on the subject, I have not been able to obtain a nearer correspondence, owing most probably to the imperfection of the mode of analyzing a mixture of hydrogen and nitrogen gases. In the mixture of permanent gases, determined in this way, the hydrogen, it may be remarked, bears generally rather a less ratio than that of 74 to 26. I do not, however, consider this

fact as contradicting the accuracy of the proportions which you have assigned; and it appears to me that a sufficient reason may be given for the want of a more perfect coincidence between results, obtained by such different methods of investigation. In the products of the electrization of ammonia, the hydrogen composes nearly three-fourths of the mixture; and hence its combustion by oxygen gas is likely to be completely effected, and the whole of the hydrogen condensed into water. But after the *partial* combustion of ammonia, by oxygen gas, a residuum is left of hydrogen and nitrogen gases, of which the hydrogen usually composes less, and sometimes considerably less, than one half the bulk. In this case, it may be suspected that a small quantity of hydrogen occasionally escapes being burned; and whenever this happens, its proportion to the nitrogen will appear to be less than the true one.*

From the inflammability of a mixture of ammonia with oxygen gas, it was natural to expect that this alkali would prove susceptible of *slow* combustion. By means of a peculiar apparatus (on a plan which I have described in the *Philosophical Transactions* for 1808, part II. but on a smaller scale, and with the substitution of mercury for water), I have found that ammonia, expelled from the orifice of a small steel burner, may be kindled by electricity in a vessel of oxygen gas; and that it is slowly consumed with a pale yellow

* This consideration suggests the propriety of using no more oxygen in the first combustion of ammonia, than is barely sufficient to inflame it; or if a larger quantity has been used than is required for this purpose, and a residue consequently obtained, of which the hydrogen forms only a small proportion, it is proper to add a farther quantity of hydrogen, before the second combustion. An allowance may, afterwards be made for this addition.

flame. The combustion, however, is not sufficiently vivid to render the process of any use in the analysis of ammonia.

With nitrous oxide (containing only 5 per cent. impurity) ammonia forms a mixture which is extremely combustible. If the nitrous oxide be in excess, the proportions have a considerable range; for any mixture may be fired by electricity, of which the ammonia is not less than one-sixth of the whole. The combustion is followed by a dense cloud, sometimes of an orange colour. When the nitrous oxide greatly exceeds the ammonia, (as in the proportion, for example, of 100 to 30) there is little or no diminution after firing: and the residuum is composed of a small portion of undecomposed oxide, some oxygen gas, and a considerable quantity of nitrogen, the last of which, however, is not in its full proportion. When the nitrous oxide is further increased, still more oxygen is found in the residuum.

When, on the contrary, the alkaline gas is redundant, combustion does not take place unless the nitrous oxide forms one-third of the mixture. A little diminution takes place on firing, but no cloudiness is observed; and the residue is composed of hydrogen and nitrogen gases, with occasionally a small portion of undecomposed ammonia. As an example of what takes place, I select the following experiment from several others.

A mixture of 41 measures of ammonia, with 40 of nitrous oxide(= 38 pure), in all 81 measures, were reduced by combustion to 75, which were found to consist of 16 hydrogen and 59 nitrogen gases. To explain this experiment, we may assume (as is consistent with your own analysis*) that 100

* Researches, Res. ii. Div. 1, or Thomson's System of Chemistry, 3d edit. ii. 143.

measures of nitrous oxide, are equivalent to 52 measures of oxygen gas and 103 of nitrogen. The oxygen in 38 measures of nitrous oxide will, therefore, be 19,7, to which, when the oxygen spent in burning the residuum (viz. 8 m. is added, we obtain 27,7 for the total oxygen consumed; and multiplying by 2, we have 55,4 for the hydrogen saturated. From the residuary nitrogen (59) deduct 39 measures arising from the decomposition of the nitrous oxide + 2 m., mingled with it as an impurity = 41, and the remainder, 18 measures, is the nitrogen resulting from the volatile alkali; and as 41 measures of ammonia give $55,4 + 18 = 73,4$ measures of permanent gas, 100 would give 179 measures, in which the hydrogen and nitrogen would exist in the proportion of 75,4 to 24,6. From the same facts it may be deduced, that 100 measures of ammonia require for saturation 130 of nitrous oxide = $67\frac{1}{2}$ oxygen gas. The coincidence then, between the results of the combustion of ammonia with nitrous oxide, and those with oxygen gas, confirms the accuracy of both methods of analysis.

NITROUS GAS, which, it appears from your testimony,* does not compose an inflammable mixture with hydrogen, (nor as I am assured by Mr. DALTON, with any of the varieties of carburetted hydrogen) may be employed, I find, for the combustion of ammonia. The proportions required for mutual saturation are about 120 measures of nitrous gas to 100 of ammonia. An excess of the former gas does not give accurate results; since not only the hydrogen of the ammonia, but some of its nitrogen is also condensed; and the mixture, after being fired, exhibits the cloudy appearance usual in that case.

* Researches, p. 136.

Forty-eight measures of ammonia, being fired with 60 nitrous gas, (= 53 pure) both gases were completely decomposed; and a residue left consisting of 61 nitrogen and 9 hydrogen. Sixty measures of ammonia and 41 nitrous gas (= 36,1 pure) gave, after firing, a mixture composed of 10 ammonia, $53\frac{1}{2}$ nitrogen, and $30\frac{1}{2}$ hydrogen. But taking for granted that 100 measures of nitrous gas, according to your analysis, hold in combination a quantity of oxygen equal to $57\frac{1}{2}$ measures of oxygen gas, and of nitrogen equal to $48\frac{1}{2}$ measures, and assuming the proportions of the nitrogen and hydrogen in ammonia, to be those established by your experiments and my own. It will appear from an easy calculation, that the proportion of nitrogen, in the above residua, a little exceeds, and that of the hydrogen rather falls short of what might have been expected. I have not yet been able to reconcile these differences, by the numerous trials required in a process of so much delicacy; and I reserve the enquiry for a season of more leisure. The foregoing statement, I wish to be considered as merely announcing the general fact of the combustibility of a mixture of ammonia and nitrous gas, a property which chiefly derives importance, from its being capable of application to a new method of analysing the latter.

Before concluding this letter, I shall briefly state the results of some experiments, which I have lately made in conjunction with Mr. DALTON, on a subject that formerly occupied much of my attention; viz. the effect of electricity on the æriform compounds of carbon and hydrogen. Subsequent

reflection, as well as the candid and judicious criticisms of various writers,* have influenced me to doubt of the accuracy of a few of the conclusions drawn from my former inquiries.† The knowledge of this class of bodies has, also, been so materially advanced during the last twelve years, that the examination of their properties may now be undertaken, with much greater confidence and success than formerly. It is to be lamented, indeed, that experimentalists do not oftener retrace their labours, with the combined advantages of acquired skill, and of a more improved state of the science which they investigate.

The gases, submitted by Mr. DALTON and myself to the action of long continued electrization, were carburetted hydrogen from pit-coal of the specific gravity of about 650 (air being 1000) olefiant gas, and carbonic oxide. Each gas was used in as pure a state as possible; muriate of lime being first introduced into the same tubes in which the gases were electrified, and being withdrawn when it had exerted its full action. Platina wires were used to convey the electric discharges.

When the electrization of carburetted hydrogen or olefiant gases was continued sufficiently long, they were each found to expand, notwithstanding their extreme dryness. No carbonic acid could be discovered in the electrified gas by the nicest tests. When fired with oxygen, it gave less carbonic

* See BERTHOLLET's *Chemical Statics*, Eng. trans. Vol. II. p. 454; MURRAY's *Elements of Chemistry*, Vol. II. Note G; a letter from an anonymous correspondent in NICHOLSON's *Journal*, 8vo. II. 185; and AIKIN's *Dictionary of Chemistry*, I. 251.

† "Experiments on Carbonated Hydrogen Gas, with a View to determine whether Carbon be a simple or a compound body." *Phil. Trans.* Vol. LXXVII.

acid than the unexpanded gas, and required less oxygen for saturation. Calculating, from the diminished product of carbonic acid, how much gas had been decomposed by electrization, it appeared that the decomposed part, in all cases, was about doubled. The smaller product of carbonic acid from the electrified gas, was sufficiently explained by a deposition of charcoal on the inner surface of the glass tube, too distinct to be at all equivocal, and most abundant from the olefiant gas. No addition whatsoever of nitrogen was made by the electrization. It appears, therefore, that the hydro-carburetted gases, like ammonia, are separated by electrization into their elements, the carbon being precipitated, and the hydrogen evolved in a separate form, and acquiring a state of greater expansion. This change, however, is effected much more slowly, than the disunion of the elements of ammonia.

From a portion of carbonic acid gas, carefully dried by muriate of lime, and electrized with platina conductors, we obtained, after removing the undecomposed gas by caustic potash, a residuum equal to about one twentieth the whole gas which had been employed. It was found on analysis to consist of oxygen and carbonic oxide gases, in such proportions as to inflame on passing an electric spark through it without any addition, and to be thus convertible again into carbonic acid. In the experiments of M. SAUSSURE, jun.* that ingenious philosopher obtained only carbonic oxide by the same operation, owing doubtless to the electricity having been conveyed by conductors of copper, which would become oxidized, and prevent the oxygen from being evolved in a separate form.

Carbonic oxide, electrified with similar precautions, did not

* Journal de Physique, Tom. LIV. p. 450.

appear to undergo any change. Eleven hundred discharges from a Leyden jar had no effect on a quantity of the gas, equal to about one tenth of a cubic inch. Its bulk, after this process, was unaltered; no carbonic acid could be discovered in it; and there was no decided trace of oxygen gas in the residuum. The carbon, it appears, therefore, which exists in carbonic oxide, must be held combined by an extremely strong affinity.

With sincere esteem and respect, I am,

Dear Sir,

Your faithful and obliged friend,

WM. HENRY.